an absolute desert. In an ordinary summer's afternoon walk at home one sees more species of plants in flower than I have seen since we entered the river. . . . However, this may improve, as it is now the dead of winter here, and with the advent of spring I hope to see many new and interesting flowers appear. Zoologically, too, it is disappointing, except in the case of birds. In the lower parts of the river not a bird was to be seen, but now they are rather more frequent, and I have already observed 116 species, of which I believe about 30 have not before been collected in Argentina. Owing to the desert nature of this part of the Chaco, its human inhabitants are very few, scattered, and nomadic. We have not seen a single Indian or canoe on the Pilcomayo. But we know they are about, for nearly every day we see their great fires for hunting all around us, and we occasionally come across a chipped palm, or the remains of an old *tolda*, the rude shelter which serves them as a tent; now and again, too, we see a human footprint, sometimes of immense size, impressed upon the muddy margin of a lagoon. So we are always on the alert, the four Britons of the Expedition keeping watch at night, fully armed and wide awake. The four said Britons are Poole, Kenyon (English), Henderson the chief engineer, and myself. When I go away collecting also, as I every day, I always go with loaded revolver and knife—ready for emergencies. For in addition to Indians there are abundance of tigers about, which one has to be prepared for. Yesterday we got an alligator close to the boat, 8 feet long. The alligators here are all small, 8 feet being the largest we have seen. . . .

"As regards food we are on very short rations, being within a month or so of the end of our provisions. The canoe is to be sent down soon, I believe, to hurry up the fresh supplies of provisions, and by it I shall send this letter, although it is very doubtful whether you will ever get it. The health of the men is not good; we have always two or three of the 17 on board ill. I have, however, had excellent health. The only thing disagreeable is the fearful cold. In the mornings the thermometer is often nearly at freezing-point, and I feel quite benumbed. Fortunately, it generally gets a little warmer during the day, the temperature rising in the afternoon to between 70° and 90° F. The river-water is regular brine here, quite as salt as sea-water, and when occasionally we run out of fresh water for a few days, it is very disagreeable having to take coffee, &c., made with the salt water. Of fruits here, there are none worth eating. The young parts of the palm-trees are eatable, and we use a good deal of it in order to economize the rice, &c. I don't expect at all that we can possibly reach Bolivia, and I don't think the river could ever be made navigable."

Protective Colours.

MR. POULTON, in his book entitled "The Colours of Animals," seeking a reason for the glistening metallic colours of many chrysalides, after showing that the colour is probably protective in its origin, states "that it has arisen from the protective resemblance to rough dark surfaces of rocks."

He comes to this conclusion after failing to find other more probable examples of glistening bodies in nature.

Are such not, however, very common (i.) in the slime or mucous covering many of the Invertebrata, and which snails and slugs leave on all surfaces over which they have passed; (ii.) the webs of spiders and their allies, especially if moist; (iii.) the exudation or excretion of many plants; (iv.) decomposing bodies; (v.) the bark of many trees?

Perhaps the commonest places to find glistening chrysalides are on palings, tree trunks, and various plants; all of which structures are usually resplendent with one or more of the above metallic hues, and among which the chrysalides are very hard to find.

May not these more common objects be those of attempted resemblance, rather than the less frequent pieces of broken rock?

Grosvenor Club, Bond Street, W. WALTER K. SIBLEY.

MR. SIBLEY'S letter appears to me to contain valuable suggestions as to the meaning of the metallic appearance of certain chrysalides. It is probable that a resemblance to the objects he suggests does aid in concealing the pupæ. Mr. Roland Trimen has similarly concluded that certain brilliant beetles (*Cassididæ*) are protected by resembling drops of dew. At the same time I think that there is some evidence that the metallic appearance of the pupæ of *Vanessidæ* may have been originally acquired in order to favour concealment against glittering mineral surfaces. The evidence is as follows:—(1) In shape and character of the surface these pupæ strongly resemble a rough and broken piece of rock. (2) They appear in two forms, resembling grey and weathered as well as freshly exposed and glittering rock surfaces. (3) When they seek green leaves for pupation they either conceal themselves with the greatest care (*V. atalanta*), or a glittering variety of other species is represented by a green variety which is inconspicuous against the leaves (*V. Io*). (4) Another species (*V. urticæ*), which lacks the habit of *V. atalanta* and the green variety of *V. Io*, is, as far as my experience goes, very rarely found on the leaves of its food-plant, and when so found, is, as a rule, diseased.

I mention the chief lines of evidence upon which I have relied in order to show that it was not merely the failure "to find other more probable examples of glittering bodies in nature" which led me to adopt the view alluded to by Mr. Sibley. Although I still consider that my hypothesis is probable, at any rate for the *Vanessidæ*, I am convinced that the resemblance to other glittering objects, such as those mentioned by Mr. Sibley, has favoured the development and especially the persistence of the metallic appearance. E. B. POULTON.

September 19.

The Aryan Cradle-land.

"IT will be for the benefit of our science," said the President of the Anthropological Section of the British Association, "that speculations as to the origin and home of the Aryan family should be rife; but it will still more conduce to our eventual knowledge of this most interesting question if it be consistently borne in mind that they are but speculations." With the latter, no less than with the former opinion, I cordially agree. And as, in my address on the Aryan cradle-land, in the Anthropological Section, I stated a greater variety of grounds in support of the hypothesis of origin in the Russian steppes than has been elsewhere set forth, I trust that I may be allowed briefly to formulate these reasons, and submit them to discussion.

(1) The Aryans, on our first historical knowledge of them, are in two widely separated centres, Transoxiana and Thrace. To Transoxiana as a secondary centre of dispersion the Eastern Aryans, and to Thrace as a secondary centre of dispersion the Western Aryans, can, with more or less clear evidence, or probable inference, be traced, from about the fourteenth or perhaps fifteenth century B.C.; and the mid-region north-west of Transoxiana and north-east of Thrace—and which may be more definitely described as lying between the Caspian and the Euxine, the Ural and the Dnieper, and extending from the forty-fifth to the fiftieth parallel of latitude—suggests itself as a probable primary centre of origin and dispersion.

(2) For the second set of facts to be considered reveal earlier white races from which, if the Aryans originated in this region, they might naturally have descended as a hybrid variety. Such are the facts which connect the Finns of the north, the Khirgiz and Turkomans of the east, and the Alarodians of the south, with that non-Semitic and non-Aryan white stock which have been called by some Allophyllian, but which, borrowing a term recently introduced into geology, may, I think, be preferably termed Archaian; and the facts which make it probable that these white races have from time immemorial met and mingled in the South Russian steppes. Nor, in this connection, must the facts be neglected which make great environmental changes probable in this region at a period possibly synchronous with that of Aryan origins.

(3) In the physical conditions of the steppes characterizing the region above defined, there were, and indeed are to this day, as has been especially shown by Dr. Schrader, the conditions necessary for such pastoral tribes as their language shows that the Aryans primitively were; while, in the regions between the Dnieper and the Carpathians, and between the Oxus and the Himalayas, the Aryans would, both in their south-western and south-eastern migrations, be at once compelled and invited, by the physical conditions encountered, to pass at least partially from the pastoral into the agricultural stage.

(4) The Aryan languages present such indications of hybridity as would correspond with such racial intermixture as that supposed; and in the contemporary language of the Finnic groups Prof. de Lacouperie thinks that we may detect survivals of a former language presenting affinities with the general characteristics of Aryan speech.

© 1890 Nature Publishing Group

(5) A fifth set of verifying facts are such links of relationship between the various Aryan languages as geographically spoken in historical times, such links of relationship as appear to postulate a common speech in that very area above indicated, and where an ancient Aryan language still survives along with primitive Aryan customs. For such a common speech would have one class of differentiations on the Asiatic, and another on the European side, caused by the diverse linguistic reactions of conquered non-Aryan tribes on primitive Aryan speech, or the dialects of it already developed in those great river-partitioned plains.

(6) A further set of verifying facts are to be found in those which lead us more and more to a theory of the derivative origin of the classic civilizations, both of the Western and of the Eastern Aryans. Just as, between the Dnieper and the Carpathians, and between the Oxus and the Himalayas, there were such conditions as must have both compelled and invited to pass from the pastoral into a partially agricultural stage; so, in passing southward from each of these regions, the Aryans would come into contact with conditions at once compelling and inviting to pass into a yet higher stage of civilization. And in support of this all the facts may be adduced which are more and more compelling scholars to acknowledge that in pre-existing Oriental civilizations the sources are to be found, not only of the Hellenic and the Italic, but of the Iranian and the Indian civilizations.

(7) Finally, if the Hellenic civilization and mythology is thus to be mainly derived from a pre-existing Oriental or "Pelasgian" civilization, it is either from such pre-existing civilizations, or from Aryans such as the Kelto-Italiots, migrating northward and southward from Pelasgian Thrace, that the civilization of Western and Northern Europe would, on this hypothesis, be traced: and a vast number of facts appear to make it more probable that the earlier civilization of Northern Europe was derived from the south than that the earlier civilization of Southern Europe was derived from the north.

The three conditions of a true solution of the problem either of Semitic or of Aryan origins appear to be these. First, the locality must be one in which such a new race could have ethnologically, and secondly philologically, arisen as a variety of the Archaian stock of white races; and thirdly, it must be such as to make easily possible the historical facts of dispersion and early civilization. And I venture to submit the above sets of facts as not inadequately, perhaps, supporting the South Russian "speculation as to the origin and home of the Aryan family." J. S. STUART GLENNIE.

The Shealing, Wimbledon Common, September 22.

Mr. Dixon's Mode of Observing the Phenomena of Earthquakes.

MR. HAROLD DIXON's letter in NATURE of Sept 18 (p. 491) is exceedingly interesting to seismologists. On two occasions he was able to make the only kind of observation which is of any value unless seismographs are actually employed; he has been able to make these in England, where earthquakes are rare, and I know of no record of such definite information being given by any of the trained observers in Japan, where earthquakes are so numerous. It requires great coolness to make such observations at such a time.

Seismographic records show that even in destructive Japanese earthquakes the vertical displacement of the ground is usually less than one millimetre, so that the mere *difference* in vertical displacement observed by Mr. Dixon between two points in the same room must have approached five hundred times the greatest *absolute* vertical displacement in Japan. Mr. Dixon truly says that, if the displacement observed by him had been due to the movement of the hill itself, it must have meant a good deal, for it would have meant some hundreds of thousands of times the greatest vertical earth movement recorded by any seismograph.

When I say that Mr. Dixon's letter is interesting, I make the assumption that what he observed was not merely what anybody observes who raises his head when looking at a distant hill through a window. JOHN PERRY.

31 Brunswick Square, W.C., September 24.

Butterflies Bathing.

IN NATURE of August 21 (p. 402) is a note taken from the *Victorian Naturalist* describing an observation made by Mr. G. Lyell, Jun., of Melbourne. He states that several butterflies (*Papilio macleayanus*) were seen to enter the water backwards, remain partly submerged for about half a minute, and then fly off to the hill-sides refreshed with their bath. The heat of the weather is given as the cause of their action.

I should like to suggest that the insects were probably engaged in *depositing their eggs*. Perhaps some one who has the opportunity will ascertain if the larva of this butterfly is aquatic, or feeds on plants growing at the water's edge.

G. A. FREEMAN.

St. Olave's Grammar School, Southwark.

Surface-tension and Surface-viscosity.

IF an oiled needle be placed on the surface of pure water, it will be supported, but if it be washed in a solution of potash, it will sink. In the first case the effect cannot be due to the surface-tension, which is much diminished by the oil. Has the viscosity of the oil anything to do with it? Also in the case of a soap-bubble, is the effect due to viscosity, and not to surface-tension; and what is the difference between surface-tension and surface-viscosity? They are both, no doubt, due to cohesion, but it is difficult to form definite conceptions of the two properties. Would any of your readers kindly answer the above questions, and give references to any works bearing thereon? Maxwell's "Theory of Heat," on capillarity and viscosity, does not seem to throw any light on the matter. W. P. O.

Leicester, September 25.

ON STELLAR VARIABILITY.[1]

II.

I HAVE before stated that the variability phenomena observed in stars of the Groups I. and II. and VI. are produced by the same cause; all differences in the details of the effects being due to the different physical nature of the central body. In Groups I. and II. it is a swarm of meteorites with which we have to deal; in Group VI. it is a condensed star of low radiation surrounded by a dense atmosphere containing carbon in some combination.

In both cases the bodies are normally dim; in Groups I. and II. they are so because the meteorites when undisturbed are relatively free from collisions; in Group VI. they are so for the reason stated above, the star being on the verge of extinction.

I insist upon this dimness, because the dimmer the central body the more important becomes the luminosity caused by, or set up in, secondary swarms. Further, such variability as we are now considering is unknown in the case of the hotter stars.

It is clear that phenomena produced in either group *by the action of two swarms* should strongly resemble each other, and that if it be found that this explanation holds good in one case it should be found to hold equally good in the other. It is to be expected then that phenomena observed in each may throw light upon the other, and that the view advanced may be tested by the differences observed.

Let us consider two hypothetical cases, to start with, in Groups I. and VI.

In Group I. we have a condensing nebula the light of which when undisturbed is say 6 mag. Round this there revolves a cometary swarm say in six time units. At periastron collisions occur which raise the light of the combined swarms to 3 mag. There is also another similar swarm revolving in say twelve time units. The conditions are such that this second swarm produces a smaller disturbance which only raises the light to 4½ mag. We will assume the periods to be exactly commensurable, and the apastra to occur together. It is obvious that alternate minima will be raised by this second revolving swarm, *but the maxima will be constant.*

In order to put results of this nature into diagrammatic form we must consider that we are dealing with certain

---

[1] Continued from p. 419.

© 1890 Nature Publishing Group